

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF ZOOLOGY

September 19, 1947

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Dear Sol,

I will send you the reprints you want at once. Actually, I was not sure whether I gave you a copy at C.S.H. or not. We are just in the midst of sending out reprints now and you would have gotten yours soon anyway.

I am sure you must have had a very interesting time in Europe. I have been getting fragments of information about your experiences from others who were there and saw you. Your report on Winge and his results is interesting indeed. Of course, you know much more about the situation than any outsider such as myself and your judgment is probably correct. However, I should certainly be glad to see the multiple gene hypothesis thoroughly explored. After all, the fact that there are some data that cannot be explained does not make it preferable to the cytogene theory for which there also seem to be data difficult to explain.

I admit I am very bewildered as to how you and Lindegren stand on the melibiose case. After talking to you at C.S.H. I got the impression that Lindegren had given up the cytogene and had gone all out for classical genetics. Subsequently, I have spoken to others ~~and~~^{who} talked with Lindegren later and from them I got the impression he is still trying to save the cytogene theory. Please let me know how things do stand and why, for I am about to set out on a Sigma Xi lecture tour and I am sure the yeast work will come up time and again. I would, therefore, appreciate enormously having a letter, setting forth the present status of the melibiose case and how you and Lindegren each interpret it, and particularly whether there is any new evidence on it since Lindegren's C.S.H. paper.

I shall also have to discuss plasmagenes in my lecture and I must say frankly that I am not sure you will approve of my conception of the history of the case. I have traced the theory back nearly 50 years and feel that the evidences for it go back almost as far. In view of the considerable publicity you and Kamen got in connection with this theory, I should like very much for you to tell me what you consider to be your distinctive contribution to the theory and to the evidence for it. I don't want to do anyone any injustice in my presentation; hence, I would appreciate your setting me straight. As I leave here October 2nd, the sooner I can hear from you the better from my point of view.

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It is good to know that you are back to work again, and I am sure you will have your usual success and good luck.

Our current work, besides pursuing kappa and paramycin further, is mainly centered on the genetics of antigenic characters. Results are coming in very rapidly and I suspect the current story will be more interesting even than the kappa story. I hope to have a chance to talk it over with you. I will be at Columbia, talking to the Sigma Xi group there on October 3rd and will spend all day Saturday there. I will also be in St. Louis, lecturing at St. Louis University on Monday, November 10th, but will be in the city Saturday morning, November 8th, and will not leave until Tuesday morning early. I hope you can arrange to look me up at one place or the other so that we can talk things over.

With best regards,

Cordially yours,

Tracy

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